MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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WARREN DE LA RUE, Esq., President, in the Chair.

Capt. Basevi, Royal Bengal Engineers;
Albert Escott, Esq., Royal Hospital Schools, Greenwich;
H. P. Finlayson, Esq., Dover; and
R. S. Newall, Esq., Ferndene, near Gateshead,
were balloted for and duly elected Fellows of the Society.

On the Satellite of Sirius. By M. Otto Struve.

The following observations of the satellite of *Sirius*, discovered by Alvan Clark, have been made with our large refractor:—

	Power.	Distance.	Position.	Notes.
		"	°	
1863, March 16	307	10·30	79·8	Distinctly seen.
18	,,	9·99	81·2	Good observation. Satellite estimated 8 mag.
1864, March 6	307	11·22	75·2	Good observation. Satellite hardly 10 mag.
19	,,		76·6	Bad images; uncertain.
21	,,		74·7	Uncertain.
22	,,		74·3	Tolerably good, through clouds.
28	,,	10·80	75·6	Good observation. Satellite at least 9 mag.
28	412	10·73	74·3	

Hence we have for the mean of the good observations,

1863·21	ρ = 10·14	P = 80·5
1864·22	10·92	74·8,

and, consequently,

The annual change of distance	$+0''\cdot77$
,, ,, position	$-5^\circ\cdot7$.

According to Mr. Safford's computations, the hypothesis that the small star is in no physical connexion with *Sirius*, and has for itself no sensible proper motion, demands for the same time

An annual change of distance	$+0''\cdot89$
,, ,, position	$-5^\circ\cdot8$;

while the hypothesis that the small star was identical with Bessel's obscure disturbing body, would imply a feeble diminution of distance, and also a diminution (but only of $1^\circ\cdot4$) in the angle of position for the same interval.

On the first glance we are, therefore, led to the conclusion, that the hypothesis of accidental juxtaposition of the two stars is by far the more probable; and this conclusion is even strengthened by the comparison of Mr. Bond's observations of 1862, by which we have,

$$1862\cdot2 \qquad \varrho = 10''\cdot07 \qquad P = 84^\circ\cdot6$$

Nevertheless, I do not yet regard this conclusion as sufficiently established. In my opinion a very strong objection against the accidental juxtaposition must be derived from the circumstance that William Herschel, at the end of the last century—when, with this supposition, the small star ought to have had a distance of about one minute from *Sirius* to the south-west—never has noticed its existence, though it is well known that about that time he has frequently observed *Sirius* as a test-object for the quality of his mirrors. To account for his not having seen the small star, we should be compelled to admit a considerable variability of its light; but though some variability is indicated by our observations, it must be conceded that in this case the estimation is very much influenced by the state of the atmosphere, particularly in our latitude, where *Sirius* in its culmination attains an elevation of only $13^\circ\cdot5$.

On this occasion it might be remarked, that, if the light of the small star is constant, it is certainly not much below the 8th magnitude, as it was estimated 1863, March 18th. In this respect our observation of 1864, March 28, is quite decisive: we could see the small star, notwithstanding its low altitude, quite distinctly a very few minutes after sunset, at a time when other stars of the 9th magnitude could only be discerned with some difficulty in higher elevations.

On the other hand, it appears difficult to account for the

differences in the two years' measures, if really the small star should be the obscure body. Accidental errors cannot have produced these differences, at least not in the positions, which, according to the agreement of the observations made on different days, are subject to probable errors only of a few tenths of a degree; also the systematic errors (*Monthly Notices*, 1860) to which my micrometrical measures are subject, would not help to explain these differences, as they affect the measures of both years by the same quantity (about $+2^{\circ}\cdot0$). Therefore we are led to suppose the existence of constant errors of another nature for each year, but varying from one year to the other. The possibility of similar errors is indicated by some measures made on the 28th March by Dr. Winnecke. After having carefully adjusted the focus for his eye, he has found that day $P = 79^{\circ}\cdot7$, exceeding by $4^{\circ}\cdot7$ the result of my observations.

Perhaps this discrepancy might be explained by the very sensible prismatic image exhibited by *Sirius* in the low altitude in which it has been observed here, combined with a different sensibility for colours of the eyes of the two observers. In fact, if our judgment on the centre of the large star varies only by half a second, it would produce a change of nearly 3° in the angle of position of the small star; and the remaining difference of our measures ($1^{\circ}\cdot7$) would be fully explained by the above-mentioned systematic errors of my measures. Admitting that this suggestion is right, it is not yet sufficient to account for the differences of my own measures in both years, if we do not make the additional supposition that the sensibility for colours has varied with me from one year to the other. Whether that be the case or not, I have no other indications, and therefore must suspend the judgment on the physical connexion or merely optical juxtaposition of the small star until next year.

Pulkowa, April 28, 1864.

Mr. Dawes remarked that he had seen the satellite twice only, although he had sought for it several times during the last year. He believed the very best atmospheric conditions were necessary to detect it. The last time he had been able to see it, he had looked for it soon after sunset, and watched it for a considerable time, until he lost sight of it; and he was really surprised to see in what a strong twilight it was visible; he conceived it was not so very small in itself, but only appeared so in consequence of its proximity to the prodigiously bright star of which it was the companion, and he believed also that if the star were higher we should see it

without any difficulty at all. He thought M. Goldschmidt's observations proved this.

Mr. Hodgson said he had seen it more than once on very fine nights with a 6-inch aperture.

On the Probable Error of a Meridional Transit-Observation, by the "Eye-and-Ear" and Chronographic Methods. By Edwin Dunkin, Esq.

In the *Monthly Notices* (vol. xx. p. 86) a short paper of mine is inserted, in which I have given the results of an investigation which was undertaken for the purpose of ascertaining, from the most trustworthy sources, the difference between the mean probable error of a meridional transit-observation made by the ordinary method of "eye-and-ear," and that of a similar observation made by the galvanic or chronographic method. The fundamental numbers were taken from the transits observed with the transit-circle at the Royal Observatory in the years 1853 and 1857 respectively, as published in the *Greenwich Observations* for these years.

An elaborate and important paper on the same subject by the late Dr. C. F. Pape, was subsequently published in the *Astronomische Nachrichten*, Nos. 1284–1286. This interesting communication gave very similar results to those exhibited in my paper, so far as the two were in common.

Very lately, however, my attention has again been directed to these researches by the questions of some visitors at the Royal Observatory. On one or two occasions I have been asked, "What is the difference between the probable errors of transits of stars of the first and sixth magnitudes?" My impressions, as a transit-observer of more than twenty years' experience, have certainly led me to the conclusion that observations of stars of moderate magnitude only, such as the fifth and sixth, convey at the moment of transit the greatest satisfaction to the observer's mind, whether the observation be made by "eye-and-ear" or recorded on the chronographic register; and further, I have also considered that, for exceptional purposes of observation, when extreme accuracy is required, the few brightest stars should, if possible, always be excluded in the night-time from the observing list, on account of the frequent difficulty of estimating the exact centre of the image of the star, which on most occasions has the appearance of a blurred mass of light. This bad definition is very evident on those nights when, from local and other causes, the atmosphere is in a disturbed condition. Similar impressions to the above have probably been made on the minds of most observers

who have been accustomed to regular meridional transit-observations.

The reader will see, however, by the following inquiry, that these impressions, so far as the resulting accuracy of the observation is concerned, are not strictly confirmed, and that the discordance of a single clock-time from the mean of all the observed clock-times, is in reality nearly equal for stars of all magnitudes as far as the sixth.

In the first place to satisfy my own mind, and secondly because this subject is always interesting to practical astronomers, I determined to extend my former investigation, so as to include a greatly increased number of transits observed by the two methods, and to arrange them into six divisions, according to the magnitudes of the stars. In selecting the necessary transits, I have advisedly rejected those of the largest stars when observed whilst the Sun was above the horizon. The stars of the first magnitude used are *Aldebaran*, *Rigel*, *α Orionis*, *Sirius*, *Procyon*, *Regulus*, *Spica*, *Arcturus*, and *α Aquilæ*. Those of smaller magnitude are too numerous for special mention: they have been chosen without partiality for any particular stars, excepting that, to save some computation, those have been preferred which had been the most frequently observed. All the stars are included within 30° of the equator.

The process of reduction may be thus explained briefly:— 1. The "eye-and-ear" transits have been extracted from the *Greenwich Observations* for 1853, Section "Transits Observed." To the clock-time recorded at each wire, and which is to be found in the printed volume of *Observations*, a correction has been applied corresponding to the value of the interval between that wire and the mean of the wires. These corrections for the intervals have been specially computed for each star, from the equatoreal intervals published in the Introduction to the *Greenwich Observations*. By the application of the computed intervals to the observed clock-times, I have obtained for each transit seven computed clock-times, all being referred to the mean of the system of wires. A mean of the computed clock-times has then been taken, and the difference of each from the mean found. These differences, or discordances, have then been treated as in my former paper by the formulæ explained in *Calcul des Probabilités et Théorie des Erreurs*, by M. Liagre, Sections 105 to 113. 2. The chronographic transits have been extracted from the *Greenwich Observations* for 1857, except those observed by Mr. Henry, which were taken from the volumes for 1854 and 1855. Mr. Henry's chronographic observations, however, are not included in the following tables, except in that exhibiting the probable errors of the transits of different observers. The method of treatment of the chronographic transits over the nine wires is similar to that adopted for the "eye-and-ear" transits over the seven wires. The

number of transits under discussion was limited to 100 for each magnitude, making, including both methods, a total of 1200 transits.

The following table contains the mean probable error of a transit over *one wire*, and also of a *complete transit*, observed according to both methods. The separate errors have been classified into six divisions, as previously explained; the numbers inserted in the table being the mean of the errors deduced from the observations of each group.

Magnitude of Stars.	Probable Error of a Transit over *One Wire*.		Probable Error of a *Complete* Transit.		Number of Transits over *One Wire*.		Number of Complete Transits by each Method.
	Eye-and-Ear.	Chronographic.	Eye-and-Ear.	Chronographic.	Eye-and-Ear.	Chronographic.	
	s	s	s	s			
1	±0·074	±0·053	±0·028	±0·018	700	900	100
2	0·079	0·050	0·030	0·017	,,	,,	,,
3	0·076	0·052	0·029	0·017	,,	,,	,,
4	0·082	0·052	0·031	0·017	,,	,,	,,
5	0·078	0·049	0·030	0·016	,,	,,	,,
6	0·078	0·051	0·029	0·017	,,	,,	,,
Mean Probable Error, and Total Number of Obs.	±0·078	±0·051	±0·029	±0·017	4200	5400	600

By arranging the separate numbers from which the mean values in the second and third columns of the preceding table are derived, into a series of new groups or divisions according to the N.P.D. of the stars, but without regard to their magnitude, we have the following resulting probable errors:—

N.P.D.	Probable Error of a Transit over *One Wire*.		Number of Observations at *One Wire*.	
	Eye-and-Ear.	Chronographic.	Eye-and-Ear.	Chronographic.
° °	s	s		
60 to 70	±0·089	±0·052	651	828
70 to 80	0·082	0·052	1358	1512
80 to 90	0·073	0·051	1169	1512
90 to 100	0·070	0·051	679	558
100+	0·070	0·050	343	990
Mean Probable Error, and Total Number of Observations ...	±0·078	±0·051	4200	5400

A sensible increase is shown in the probable errors of "eye-and-ear" transits as the N.P.D. decreases, as might be

expected. The chronographic transits also exhibit a slight tendency to a similar increase, but its amount is scarcely sensible.

The values given above have been determined from the separate probable errors of transits observed by a considerable number of observers — the majority, however, by the established assistants of the Royal Observatory. It is important and interesting, therefore, again to analyse the separate transits, and to find the mean discordance due to each of the principal observers. The discordances of the computed clock-times, found as previously explained, are not necessarily of larger amount in the transit of any observer whose general method of observing may differ considerably from that of the remaining observers; for it is proved by the following comparison-tables that, though an observer's ordinary personal equation may be of a very different value from that of another, yet the general accuracy of the transit-estimations is equally good in both cases. In skilled transit-observers the ordinary personal equation is usually so constant from year to year, as to make us infer that the same method of observation is continued by the same observer for a long period. There are, however, occasional exceptions to this steadiness of observing, which can be illustrated by noticing the change in the personal equation between Mr. Henry's and Mr. Rogerson's "eye-and-ear" transits, which gradually varied, in the years 1846 to 1853, from $0^s.31$ to $0^s.67$. The annual change of this value was very remarkable, and was proved to depend in a great measure on a corresponding annual alteration in Mr. Rogerson's method of observing.

To exhibit the yearly uniformity in the methods of observation of the chronographic transits, I have only to extract the "personal equations" of the present principal observers at the Royal Observatory, from the *Greenwich Observations* of the last three years. The observers are, Mr. Stone, Mr. W. Ellis, Mr. Criswick, Mr. Carpenter, and myself. The numbers have been referred, as is usual at Greenwich, to my method of observing, and have been determined from a comparison of the transits of clock-stars observed during the respective years.

	1861.	1862.	1863.
	s	s	s
Dunkin—Stone	−0·13	−0·14	−0·14
Dunkin—Ellis	−0·13	−0·15	−0·16
Dunkin—Criswick	+0·15	+0·15	+0·16
Dunkin—Carpenter	+0·09	+0·08	+0·07

To determine the probable personal discordance of a transit over one wire, I have classified the separate errors into groups according to each of the principal observers. The few occasional transits made principally by the supernumerary com-

puters are placed together in one miscellaneous group. In addition to these, I was desirous to obtain the necessary materials, so as to include Mr. Henry's chronographic observations in this comparison; but in consequence of the death of that gentleman in 1856, I was obliged to extract his transits from the volumes for 1854 and 1855. These transits are therefore supplementary to those which form the basis of the preceding tables. The results are given in the following tables for the six divisions relative to the stars' magnitudes. The mean value for each observer has been found by giving to each divisional result a weight proportional to the number of transits observed over one wire in each division, the number of observations being always considerable.

"*Eye-and-Ear*" *Observations.*

Stars' Magnitude.	Probable Error of a Transit over *One Wire.* Henry.	Rogerson.	Dunkin.	Henderson.	W. Ellis.	Various Observers.
	s	s	s	s	s	s
1	±0·103	±0·062	±0·058	±0·081	±0·074	±0·082
2	0·121	0·051	0·069	0·077	0·071	0·083
3	0·103	0·048	0·062	0·063	0·071	0·096
4	0·109	0·094	0·059	0·076	0·078	0·093
5	0·114	0·056	0·062	0·073	0·061	0·094
6	0·109	0·084	0·061	0·072	0·059	0·086
Mean Probable Error	±0·112	±0·065	±0·062	±0·074	±0·069	±0·089

Chronographic Observations.

Stars' Magnitude.	Probable Error of a Transit over *One Wire.* Henry.	Dunkin.	W. Ellis.	Criswick.	Lynn.	Various Observers.
	s	s	s	s	s	s
1	±0·059	±0·049	±0·058	±0·048	±0·054	±0·056
2	0·059	0·046	0·051	0·044	0·054	0·055
3	0·058	0·047	0·057	0·049	0·052	0·055
4	0·059	0·053	0·053	0·047	0·053	0·059
5	0·054	0·047	0·051	0·046	0·050	0·057
6	0·055	0·049	0·049	0·052	0·048	0·072
Mean Probable Error	±0·058	±0·048	±0·053	±0·048	±0·052	±0·060

A comparison of the two preceding tables exhibits more clearly than any other the great advantages which astronomy will derive from the substitution of the chronographic method

of recording transits at the Royal Observatory, in the place of the original method of estimation by "eye-and-ear." The great steadiness of the various probable errors in the chronographic transits is something remarkable. This great regularity of transit-observing is very evident, not only from an examination of the values given for the different star-groups of the same observer, but also by comparing those resulting from the observations of the various observers. There is no need, however, to offer any further remarks, as the tables sufficiently explain themselves. But it is interesting to notice that, by the introduction of the chronographic system of registration, the probable error of Mr. Henry's transits was reduced to one-half the amount of that resulting from the original or "eye-and-ear" system of observation.

The numbers which I have hitherto discussed in this paper refer solely to the errors of the actual transit. In chronographic observations these numbers may be composed of three different kinds of error: first, the personal discordance due to a hasty or tardy galvanic contact made at the moment of the star passing the wires in the field of view; secondly, the error of transcribing from the barrel-sheet of the chronograph; and thirdly, the error due to an erroneous value of the intervals between the wires used in these reductions. Of these three sources of error, the two first are, without doubt, the causes of the discordances between the results for each wire and the mean of wires. With respect to the third possible source of error, which is common to both methods, the intervals of the wires are always determined with so much care that it may be safely assumed that their values, so far as this investigation is concerned, are sensibly correct for both systems of wires. In the "eye-and-ear" transits the discordances must therefore rest entirely on the estimations of the observer.

The subject would be incomplete were I not to include in this investigation the determination of the probable error of a Greenwich result for Right Ascension. This has been performed by extracting the separate results, as recorded in the "Ledger," where the mean Right Ascensions are given for January 1. In this instance the probable errors are a combination of not only the errors described above, but also those of any erroneous application of the collimation, level, and azimuth errors, clock-errors, &c. But as these special sources of error must be supposed to affect in a similar manner the mass of the observations by the two methods, the *difference* between the probable errors of the results for Right Ascension deduced from the "eye-and-ear" and chronographic transits should agree with the *difference* between the corresponding errors of a complete transit given in a preceding table, which will be seen is sensibly the case.

For my present purpose I have simply re-arranged the original numbers, the mean results whereof are exhibited in

the table on page 88 of the *Monthly Notices*, vol. xx., into six divisions. By this means I have obtained the probable error of a result for Right Ascension for different classes of stars, as observed at the Royal Observatory in the years 1853 and 1857.

Stars' Magnitude.	Probable Error of a Result for Right Ascension.		Number of Observations.	
	Eye-and-Ear.	Chronographic.	Eye-and-Ear.	Chronographic.
	s	s		
1	±0·059	±0·040	151	168
2	0·046	0·035	205	139
3	0·048	0·033	373	247
4	0·046	0·033	258	256
5	0·052	0·033	132	148
6	0·044	0·035	140	234
Mean P.E. and Total Number of Obs....	±0·048	±0·034	1259	1192

In this table the probable errors of the first group appear to be slightly in excess in both methods. I think this may be accidental, and arises probably from some of the other sources of error, rather than from the stars' magnitude.

In laying before the Society the preceding abstracts, which are the results of a great mass of computation, I consider that I can with some confidence offer the following conclusions, which are derived from this discussion:—

1. In "*eye-and-ear*" observations, the probable error of a Greenwich transit observed in 1853 over *one wire* is $\pm 0^{s}\cdot 078$, while that of a *complete* transit over the seven wires is $\pm 0^{s}\cdot 029$. In *chronographic* observations, the probable error of a Greenwich transit observed in 1857 over *one wire* is $\pm 0^{s}\cdot 051$, and that of a *complete* transit over the nine wires is $\pm 0^{s}\cdot 017$.

2. There does not appear to be any certain difference in the probable error of transits of stars between the first and sixth magnitudes.

3. In "*eye-and-ear*" transits, for stars whose N.P.D. is greater than 60°, it would seem that the probable error of a transit increases slightly as the N.P.D. decreases; while in the *chronographic* transits the corresponding changes are insignificant.

4. In "*eye-and-ear*" transits, the personal discordances are liable to a considerable variation between the different observers; in *chronographic* transits, the differences between the observers are comparatively small. The general steadiness of observing by the latter method is very remarkable, and shows the great advantage obtained by its adoption.

5. The probable error of a Greenwich result for Right Ascension in the year 1853, as determined from "*eye-and-ear*"

transits, is $\pm 0^s.048$; while the corresponding probable error resulting from *chronographic* transits, in the year 1857, is $\pm 0^s.034$. By arranging the separate results into groups, according to the stars' magnitude, it is found that no certain difference can be distinguished in the accuracy of the observed transits, excepting only that there is a tendency in both methods towards an increase in the probable error when transits of stars of the first magnitude are observed. The amount of increase in this instance, however, is in reality small, and is probably to some extent accidental.

22 *Royal Circus Street, Greenwich,*
April 18, 1864.

Mr. De La Rue remarked that it was impossible to listen to papers such as the foregoing one without congratulating ourselves upon the exactness of modern observation, and upon the industry of our lovers of science, both amateur and professional. Looking to the result of the calculations contained in Mr. Dunkin's paper, one was almost overcome by the amount of persevering industry which must have been required for bringing observations to such a point of excellence. He then referred to an improvement upon a proposal, made some time ago by Professor Wheatstone, for increasing the accuracy of transit observations. Most of the members present would be aware that there was a personal equation existing even in the chronographic system. The finger might move a little too soon or too late upon the tappet, and the tappet might hang fire, and a number of other circumstances might occur. This, Professor Wheatstone proposed, should be remedied by a system of wires in the transit instrument, which, when the star was brought between them, should follow its movement; and when the star passed the optical axis of the instrument, or any number of known points from that axis, then the chronographic contact would be made by the wires making contact, so that a number of records would be obtained independent of the will of the observer, and only dependent upon the irregularity of the movement in following the star — a method very similar to that employed by the speaker in his first lunar-photographic experiments.

Col. Strange, in a recent visit to Paris, had consulted the astronomers at the Imperial Observatory as to the value of the chronographic method, and had received a report very decisive against it, a very strong opinion having been expressed that it did not exceed the old method in accuracy, even if it came up to it. M. Le Verrier's remark in addition being, "*Cela fait de mauvais astronomes.*" He was however still of the opinion

that the Greenwich method was the more sound of the two. He had therefore, with the sanction of the Government, ordered a complete chronographic apparatus for the use of the Indian Survey. He believed that M. Le Verrier's condemnation of this process must have been based upon a comparatively small number of observations; whereas, where we have to deal with the fractional part of a second, a few observations were of very little value. The French astronomers performed their experiments no doubt with their usual skill, but they had not the advantage of an accumulation of data so extensive as that which the Society had just had laid before them. With reference to what had fallen from the President, Col. Strange remarked that he believed the method proposed by him of having a telescope so constructed that the star should automatically be followed over the wires had been already achieved: he had examined, while in Paris, an apparatus of this description, invented by M. Redier, in action, at the Paris Observatory; the wire was carried so steadily across the field — so exactly with the same velocity as the passage of the star — that the intersection of that star with the wire was a matter of the most perfect ease and certainty. In fact there was ample time for the observer to call an assistant to verify the observation before leaving the instrument. This system, together with the chronographic method, could not fail to reduce to within even smaller limits, those already excessively minute errors with which Mr. Dunkin had so ably dealt in his paper.

Mr. Walker said he had long been convinced that electricity would render very great service to astronomy, and his predictions had been verified by what they had just heard from Mr. Dunkin.

Mr. Dunkin, in reply to Colonel Strange's allusion to the objection urged by some astronomers to the chronographic system, that it tended to produce bad eye-and-ear observers, remarked, that so far from that being the case, it was found in practice that the old observers at the Royal Observatory, who had not practised eye-and-ear observations for ten years, when the clock was out of order, and they had recourse to the old method, made their observations just as accurately as they did ten years ago.

Mr. Pritchard said that perhaps few people had an idea how very small the present errors in observations really were. The whole extent of them was simply this: the third of a second of an arc might be represented by putting a human hair at the distance of 125 feet.

The following paper was then read by Mr. Dawes:—

Results of some recent Observations of the Solar Surface, with Remarks. By the Rev. W. R. Dawes.

During nearly the whole of last year and the first three months of this, the state of the air was rarely sufficiently good to permit a successful scrutiny of the solar surface with high powers on a large telescopic aperture;—on such occasions, at least, as I was able to make much use of. But on three or four days in April I found that vision was remarkably steady and distinct, and I improved the opportunities to the utmost.

As it was of considerable importance that the same minute portions of the surface should be examined and compared together under various magnifying powers, and with different shades of dark glass, I preferred, after a careful comparison with the ordinary eye-pieces and an excellent transparent diagonal, to employ a solar eye-piece of my own construction. And I would take this opportunity of correcting an idea which has been advanced, that the diffraction of the rays at the edge of a small aperture in that eye-piece interferes with the distinctness of the object viewed. Close to the edge of the field there is, of course, as Mr. Huggins well expressed it at the last Meeting of the Society, a narrow ring of diffracted light: but the breadth of this ring does not, I think, exceed 2″ on my telescope; and the smallest aperture in the diaphragm-plate has a diameter of 15″. Consequently there is a central space of at least two-thirds of the diameter in which the definition is perfect, even in the smallest aperture of the eye-piece. Yet, for the purpose now referred to, I rarely use a field of much less than *one minute* in diameter; and any object not exceeding 45″, when placed in the centre of the field and kept there by the driving-clock, is recognised with certainty under different powers, and seen with the utmost possible distinctness.

1. One of the objects I had principally in view was, to ascertain whether in any part of the photosphere any objects could be found which could reasonably be compared to willow-leaves in their form.—It may be well to state here that I have always found it difficult to devise any appropriate appellation for the small bright irregularities of the surface, which would avoid an assumption of their character, or ascribe to them a regularity of form they do not possess. In my first paper I expressed my strong objection to any *name* of this kind, as calculated to convey an erroneous impression. The term *willow-leaves* seemed utterly inapplicable to anything I had ever succeeded in discovering. A far less objectionable term, as it appears to me, is that of *rice-grains* applied by Mr. Stone to those objects with which all careful Sun-observers must be acquainted, as there is no difficulty in seeing them in a moderately favourable state of the air, and which have been familiar to myself for many years;—so much so, indeed, that

when they were not discernible I invariably abstained from any further scrutiny of the solar surface as being useless. Yet even this appellation conveys the idea of uniformity of shape and size which these objects do not possess, and is, I think, on that account objectionable. But I have been led by it to apply the term *granulations*, or *granules*, which assumes nothing either as to exact form or precise character; and I venture to hope that the term will be generally adopted.

To proceed then with the results of my recent observations.

Various portions of the surface were diligently examined with powers from 131 to 407. The bright little granulations were easily seen in all parts not very near the edge of the disk. Their forms and sizes were carefully noted, and found to be so various as to defy every attempt to describe them by any one appellation or comparison. But, as I have observed in a former communication, the rarest of all forms was the long and narrow; and in no instance did I succeed in finding one which could with propriety be compared to a willow-leaf. Occasionally, some that were nearly in contact differed so greatly in size that one was four or five times as large as the other: and while, in a remarkably bright mass, one somewhat resembled a blunt and ill-shaped arrow-head, another, very much smaller and within 5″ of it, was an irregular trapezium with rounded corners. The more quiet and perfect the views I obtained of them, the more convincing did the evidence appear that they were not individual and separate bodies of a peculiar nature, but only different conditions as to brightness or elevation of the larger masses forming the mottled surface; just as the brighter portions of that surface, and the *faculæ* also, are different conditions of the general photosphere. In these researches I met with nothing which had the slightest resemblance to the *interlacing* which Mr. Nasymth has so clearly described and so distinctly depicted in his communication to the Manchester Literary and Philosophical Society; of which letter I was not aware when I penned my former paper on this subject. Mr. Nasmyth having subsequently favoured me with a copy, I was so struck with the clearness and decision of his assertions that I began to think I must have overlooked the peculiar appearances of the objects which he has depicted in his diagram as being "*the exact form of these remarkable structural details*," which he describes as "*forming the entire luminous surface of the Sun;*" and, therefore, to leave nothing untried, I collected all the information I could as to the means employed by him and by other observers who had seen something of the same kind, in the hope that some change in my apparatus or mode of using it might at length render me successful. Now, however, it appears that Mr. Nasmyth has withdrawn his former statements as to the exact and uniform figure of the objects he claimed to have discovered on the entire surface; and that, in fact, all that might have

been regarded as a *discovery* resolves itself into an appearance perfectly well known many years before.

The darker or shaded lines between the granules I distinctly observed in many places to be pretty thickly covered with dark dots, like *stippling with a soft lead-pencil;* and these are what have been called "*pores*" by Sir John Herschel, and "*punctulations*" by his father. Some of these were almost black, and looked like excessively small spots just breaking out. But none of them were seen to enlarge or materially alter their form, though at times so sharply defined with powers 276 and 407 that it was obvious they were in general not quite round.

It appears to me quite incomprehensible that, under such definition, any bright objects of peculiar form should have escaped detection when specially looked for; and I was certainly struck with the extreme rarity of a long and narrow shape among the hundreds of granules which I examined in those four days' observations. If therefore there are any willow-leaf-shaped objects to be seen, they must be quite distinct from these granulations, and can form no regular or usual ingredients in the composition of the solar surface.

On the *penumbræ* of the spots then visible I found several long and narrow bright lines, extending like bits of white thread completely across the whole breadth of the penumbra without any break whatever; and, though there were several smaller pieces, not one of them would have suggested to my mind the comparison with a willow-leaf. Perhaps the bright granules of the general surface may sometimes be compressed into a longer form under the influence of the same forces which produce the longer threads or *straws* on the penumbra; but one of the most striking features of that part, as well as of the general surface, is, according to my observations, the entire absence of uniformity in the brighter portions with respect both to their size and shape.

2. A second object of my investigations was to determine whether the granulations existed equally in the brightest and in the less luminous portions of the surface, which together form the general and comparatively *coarse* mottling of the photosphere.

On comparing together in the same field of view several of the more and less luminous masses which produce the coarse mottling of the surface, I came to the conclusion that the granules are generally *larger* as well as brighter on the brightest parts than on the darker; the difference in brightness of the individual granules in each part being much the same as in the different masses themselves. One fact struck me as very remarkable, that, on each of these larger masses, the individual granules are all very nearly of *equal* brilliance throughout the mass to which they belong. They are not in general, if ever, mixed together, some much brighter and

others far less bright on the same mass. Occasionally I have noticed that some of the brighter masses are decidedly brighter than others of the same class, and that such have extended to a much greater length than usual. The granulations on them are also generally, I think, of uncommon size, and the shaded lines between them of a lighter tint. I strongly suspect that these specimens, if near the Sun's edge, would be seen as *faculæ*.

3. A third principal object I had in view was, to scrutinise especially the brighter parts immediately surrounding the penumbræ of spots (which in a former paper I have referred to as indicating a heaping up of the photosphere by some eruptive force proceeding from the centre of the spot), with the purpose of ascertaining whether the granulations elsewhere easily visible were to be found there; and also to examine the *faculæ* of various forms, sizes, and degrees of brightness, to determine whether any such granulations were visible upon them.

With reference to the reality of this brighter region surrounding all the considerable *profound* spots, and also some of the smaller ones, I have lately been much gratified by the corroboration of the fact afforded by some solar photographs taken by our President, and obligingly presented to me since the publication of my former paper. While the general mottling of the surface is well brought out in them, they show that this mottling never extends to the margins of the spots; which precisely agrees with my observations. In this locality I have never been able to detect any of the granulations so abundant and so easily seen elsewhere. My recent observations fully confirm the conclusion I have formerly arrived at, namely, that the commotion, of whatever kind it might be, had produced the effect of heaping up the material of the photosphere; and had confounded all the distinctions elsewhere seen of the large brighter and shaded masses, and also of the granulations on both, and the pencil-lines, occasionally stippled, by which the individual granules are distinguished.

Precisely the same results attended my examinations of the *faculæ*. Nothing resembling granulations could be found on any of them, whether they were the tortuous, thread-like objects seen near the east and west borders, or the shorter lumps of apparently similar composition near the poles. Yet the granulations were discernible in several places nearer to the Sun's edge than the faculæ were situated; so that it could not arise from the obliquity of the view that they were not visible on the faculæ themselves. I conclude therefore, that the same disturbance which produces the elevated ridges confuses the minute features elsewhere seen; and that, though there may be some traces of granulation when the faculæ are viewed almost perpendicularly, yet this is entirely lost when their sides only are seen near the Sun's limb. In this position,

however, there are often distinct evidences of irregularity in the elevation of different parts of the ridge; and these may, perhaps, when viewed perpendicularly, produce variations of brightness, like the granules of extraordinary size mentioned above.

Hopefield Observatory, Haddenham, Bucks,
1864, May 9.

On the Appearances of the Sun's Disk.

By C. G. Talmage, Esq.

I should have ventured to communicate the contents of this paper to the Society at an earlier period, had I not felt some diffidence in saying that I have never been able to see those objects which are so positively declared to exist by persons who may be considered authorities on such matters; but when it is stated by a gentleman who possesses the great experience and acute vision of the Rev. W. R. Dawes, I feel a little more confidence in giving the result of my own observations.

Since 1861, November, I have most carefully searched for Mr. Nasmyth's "willow-leaves," under various circumstances.

From 1861, November, to 1862, April, I was engaged in astronomical pursuits at Nice, and on every fine day I examined the Sun with one of three Equatoreals—a 4-inch, a 6-inch, and an 8-inch (belonging to Mr. Coventry)—with powers from 40 to 500. In the summer of 1862 I examined the Sun at Paris with another 8-inch object-glass of exquisite definition. In November of the same year I revisited Nice; and until my return, in 1863, April, I omitted no opportunity of searching for the "willow-leaves," but never have I seen the slightest trace of "willow-leaves," "rice-grains," or "thatch."

During this spring I have also examined the Sun with a small telescope, fitted with a binocular eye-piece belonging to a first-class microscope, and without any greater success, though the spots are clearly shown as funnel-shaped depressions or holes: all that I see on the Sun is merely a difference of greater or less luminosity.

A remark was made at our last Meeting that perhaps these appearances depend on focus, and the examination of diatomaceæ out of focus was given as an instance. When I returned home I examined several specimens of diatomaceæ, both easily resolvable, and tests (*Navicula cuspidata*), with various powers from 3-inch to a $\frac{1}{8}$-inch, in and out of focus; and I could compare them to nothing but diatoms out of focus; there was not

the least resemblance to anything I have ever seen on the solar disk.

Mr. Bishop's Observatory, Twickenham,
April 18, 1864.

Mr. De la Rue, referring to the full discussion which the subject of Mr. Dawes' communication had received at previous meetings, remarked that it is to the Sun himself, and to other observers, that the confirmation or non-confirmation of Mr. Nasmyth's discovery must be left. Mr. De La Rue maintains, notwithstanding what had fallen from Mr. Dawes, that it was a substantial discovery. Mr. De La Rue had recently fitted up one of Sir John Herschel's prism-eye-pieces, with a view of continuing these observations upon the Sun, and he has also designed a prism for his Newtonian Reflector, with the same view, in order to collect more facts on this subject.

Mr. Dawes said he thought one of the most remarkable things connected with the matter was that, whereas these granules, or "rice-grains," were easily seen—a small telescope, with a power of 40 or 50, bringing them into view—Mr. Nasmyth should have accepted them as his "willow-leaves," which, he says, are so difficult to see with an 8-inch aperture.

Mr. De la Rue said that, with regard to one person having observed them with a comparatively small power, he might state he could recollect the time when he could see perfectly lines and spaces of 200 to the inch; he could not do so now without a magnifier, and he thought this apparent contradiction might be accounted for in the same manner.

Mr. Stone said the appearances presented to him were what he had described, viz., luminous particles scattered over the Sun's disk, which, by way of description, he had called "rice-grains." He was not prepared to say that Mr. Dawes' "granules" were not the same things; all he could say was, that he was not aware of his having published any account of them. He felt very great confidence in the existence of what he had described, and he had examined them with magnifying powers up to 400, and had not found the slightest change in their appearance. He was not prepared to say that it might not be a very peculiar appearance of the luminous clouds, such as Mr. Dawes suggested; but he considered that, even supposing it to be so, it was well worth the attention of the astronomer.

Mr. Howlett corroborated a remark made by Mr. Dawes as to the general presence of a bright annular appearance, just outside the penumbra of a spot. He next alluded to the eye-piece recommended by Sir John Herschel for observations of the Sun by means of screen projections in a darkened chamber; the construction of which, it was remarked by Mr. Pritchard, is not at all difficult.

Comparison of the Chinese Record of Solar Eclipses in the Chun Tsew with the Computations of modern Theory. By the Astronomer Royal.

In the *Monthly Notice* for 1863, Dec. 11, our Assistant Secretary, Mr. Williams, has given an abstract of the record of thirty-six eclipses in the Chinese historical work called *Chun Tsew*. Mr. Williams has, with great care, converted the Chinese dates into dates according to the Julian calendar.

The period through which these eclipses extend is included in the time through which calculations of eclipses have been made in the French work entitled *L'Art de vérifier les Dates*. I have several times had occasion to re-calculate with great accuracy eclipses which are noted in that work (edition 1820); and I have found that, to the limits of accuracy to which it pretends, and which are abundantly sufficient for the present purpose, it is perfectly trustworthy. I have therefore made a comparison of the *Chun Tsew* eclipses with those of *L'Art de vérifier les Dates*. The result is interesting.

Of the thirty-six eclipses, thirty-two agree with those of the *Art de vérifier les Dates*, not only in the day, but also in the general track of the eclipse as given in the *Art de vérifier*, which appears to show sufficiently that the eclipse would be visible in that province of China to which the *Chun Tsew* is referred.

The eclipses

No. 10, B.C. 645, April 15,

and

No. 15, B.C. 592, June 16,

appear impossible. There cannot have been eclipses in April of B.C. 645, or in June of B.C. 592. It appears, however, from a note with which Mr. Williams has favoured me, that the date attached to No. 10 is in reality an erroneous repetition (in the Chinese mode of expressing it) of that attached to No. 11; and, in the absence of correct date, it must be rejected. In the record of No. 15 no clerical error is found, and there must therefore be error of a different class.

The eclipses

No. 22, B.C. 552, September 19,

and

No. 25, B.C. 549, July 18,

to which there is nothing corresponding in the *Art de vérifier*, are in a different category. These occur in the lunations immediately succeeding No. 21, B.C. 552, August 20, and No. 24, B.C. 549, June 19, respectively; and there is no doubt that Nos. 21 and 24, which agree with the *Art de*

vérifier, were real eclipses. Now there cannot be eclipses visible at the same place in successive lunations; because the difference of the Moon's longitudes is about 29°, and the difference of latitudes is therefore nearly 3°, which is greater than the sum of diameters of the Sun and Moon increased by any possible change of parallax for the same place. Nos. 22 and 25, therefore, were not real eclipses. It seems probable that they have been set down by the observer in his memorandum-book as days on which eclipses were to be looked for. I conjecture that the eclipses Nos. 21 and 24 were observed by one and the same person, and that he possessed science enough to make him connect the solar eclipses with the change of the Moon, but not enough to give him any idea of the limitations to the visibility of an eclipse.

There is no instance, among these eclipses, of recurrence of an eclipse at the end of a Saros; but there are eight instances of recurrence at the end of a triple Saros, namely,

No.	2	to	No.	8,	B.C.	709,	July	17,	to	B.C. 655, August 19
	8	,,		13,		655,	August	19,	,,	601, Sept. 20
	12	,,		19,		612,	April	28,	,,	558, May 31
	16	,,		30,		575,	May	9,	,,	521, June 10
	17	,,		31,		574,	Oct.	22,	,,	520, Nov. 23
	18	,,		34,		559,	Jan.	14,	,,	505, Feb. 16
	21	,,		35,		552,	August	20,	,,	498, Sept. 22
	24	,,		36,		549,	June	19,	,,	495, July 22

Royal Observatory, Greenwich,
April 5, 1864.

Occultations of Stars by the Moon, and an Eclipse of Jupiter's Third Satellite, observed at the Royal Observatory, Greenwich, in the Month of March, 1864.

Occultations of Stars by the Moon.

Day of Observation. 1864.		Phenomenon.	Moon's Limb.	Mean Solar Time. h m s	Observer.
March 18		A^1 Cancri, disapp.	Dark	6 58 30·6	J. C.
		A^2 Cancri, disapp.	Dark	7 57 27·8	J. C.
		A^1 Cancri, reapp.	Bright	9 42 54·6	J. C.
	(*a*)	A^2 Cancri, reapp.	Bright	11 3 0·8	J. C.
March 19	(*b*)	ω Leonis, disapp.	Dark	6 11 26·7	E.
		ω Leonis, reapp.	Bright	7 28 34·0	E.

Eclipse of Jupiter's Third Satellite.

March 11. Disappearance of third satellite $16^h\ 3^m\ 50^s{\cdot}7$ R.

(*a*) Probably not correct to less than a second.

(*b*) Strong daylight and much tremor. The star seemed to diminish in brightness about a second before extinction.

The initials E., J. C., and R., are those of Mr. Ellis, Mr. Carpenter, and Mr. Roberts.

Occultations of Stars by the Moon. Observed by Capt. W. Noble, at Forest Lodge, Maresfield.

Friday, March 18th. *Occultation of* A^2 *Cancri.*

The star disappeared instantaneously

at $9^h\ 30^m\ 37^s{\cdot}2$ L.S.T. = $9^h\ 43^m\ 19^s{\cdot}7$ L.M.T.

Power 115, adjusted on the star.

Saturday, March 19th. *Occultation of* ω *Leonis.*

The star disappeared very nearly instantaneously

at $6^h\ 1^m\ 34^s$ L.S.T. = $6^h\ 11^m\ 24^s{\cdot}9$ L.M.T.

and reappeared

at $7^h\ 19^m \pm 8^s$ = $7^h\ 28^m\ 46^s{\cdot}2$ L.M.T.

There was a good deal of atmospheric disturbance, and the twilight was very strong at the time of disappearance. The star was caught almost at the instant of its emergence from the Moon's bright limb.

Power 154, adjusted on the star. In each case my Ross equatoreal of 4·2 inches aperture was employed.

On the Minor Planet ⑯ *Freia.* By N. Pogson, Esq.

The following observations, by Mr. Pogson, of an asteroid, originally observed for *Concordia*, have been extracted from a letter to the Astronomer Royal, dated Madras, March 28, 1864:—

1864.	Madras M.T h m s	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″
Feb. 2	15 13 28	8 42 56·76	74 59 16·0
9	12 45 41	8 37 39·26	74 36 44·9
10	13 0 1	8 36 54·90	74 33 28·0
16	14 9 6	8 32 46·35	74 14 43·7

Magnitude 11·6.

It would appear from the calculations of Herr Theodor Oppolzer (*Ast. Nach.* No. 1471), that these are really observations of the planet ⑯ *Freia*.

Notes on α Centauri and other Southern Binaries, and on the Nebula about η Argûs. By E. B. Powell, Esq.

A brief notice of the motion, during the last two years, of *α Centauri*, may be of interest to some of the members of the Society.

In my observations of double stars, taken in 1859–62, and communicated towards the close of the latter year, the last mean measures of *α Centauri* were as follows:—

Epoch.	Position-Angle.	Distance.
1862·205	358°·0	6″·79

I have since secured the means given below:—

Epoch.	Position-Angle.	Distance.
	°	″
1863·028	1·4	7·2
1864·110	5·7	7·85

The Position for 1863 is derived from 59 observations on 6 nights; the Distance, from 36 measures on 4 nights. The separate nights did not furnish as accordant Position-results as was desirable: in fact, the latter varied between 0°·83 and 2°·2. The results for the current year are very accordant, and I have great reliance upon them. The Position is the mean of 64 measures on 7 nights, and the Distance that of 70 measures on 5 nights. The whole range of the measures for the different nights was, for Position, only ½°, and for Distance, 0″·35.

By drawing a Position-curve, it was found that the comes was on the same meridian with the primary about 1862·65; the Distance must then have been very nearly 7″.

I beg to invite attention to the important portion of the orbit now about to be described, viz. the part in the immediate neighbourhood of the lesser maximum of distance. If this maximum be accurately determined, one most prominent feature in the path will be fixed; and then, as the companion will revolve with continually increasing rapidity till its distance from the primary diminishes to 1″ or less, a really excellent orbit will be calculable in 1870, or thereabouts. I may remark that the orbit contained in the notes to my observations of 1859–62, though agreeing tolerably with observation up to this time, assigns too small a magnitude to the lesser maximum distance.

The following are my latest measures of two other Southern binaries:—

Star.	Epoch.	Position.	Distance.
		°	″
p Eridani	1863·017	250·9	4·88
γ Coronæ Aust.	1863·836	318·1	1¼ est.

The latter star is moving rapidly, though not so rapidly as was anticipated; it has described about 20° in the last four years.

In the note on η *Argûs*, in my paper already alluded to, I called attention to two points connected with the Nebula about that star: (1), that the whole Nebula had faded away very considerably in 1860; and (2), that it had altered its form, the nebulous matter having receded so as to leave open the southern end of the lemniscate vacuity. These remarks were based upon numerous observations taken in 1859, 60, 61, and 62; and I will now give a few particulars, which appear to me to possess some interest.

On the 15th April, 1860, I noted in my observation-book that the Nebula was "much fainter than formerly;" while, on the immediately preceding 23d March, the following entry occurs:—"Nebula about η *Argûs* magnificent." On the date first mentioned, η is in a rough sketch placed out of the bright portion of the Nebula, and the lemniscate is described as a "channel." Several entries follow, noting both the openness of the lemniscate to the south and the greatly diminished brightness of the Nebula. The former fact was acquiesced in by Major Tennant of the Bengal Engineers, whom I requested to examine the Nebula with the Lerebours Equatoreal of the Madras Observatory. On the 4th April, 1862, the following entry was made in my book:—"η beautifully round and clean, out of nebulous matter altogether; two patches of nebulous matter, with a passage between them, to the left or preceding."

The original object with which I turned my attention to the locality of η *Argûs* was to make micrometrical measurements of η and the neighbouring stars, for the purpose of comparing

the same with Sir John Herschel's *Cape Observations;* as subsidiary to the above, I made notes of the variations in brightness of η, but the changes in the Nebula were not dreamt of; and, in fact, I was at first inclined to fancy that they were only apparent, and due to the use of a different and inferior instrument.

I have to observe that to Mr. Abbott must be ascribed the first publication of the fact that η is no longer in the dense portion of the Nebula where it was seen by Sir John Herschel.

Madras, March 9th, 1864.

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